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A
CHEMICO-PHYSIOLOGICAL
ESSAY,

DISPROVING THE EXISTENCE OF AN ÆRIFORM FUNCTION IN THE
SKIN, AND POINTING OUT,

BY EXPERIMENT,

THE IMPROPRIETY OF ASCRIBING ABSORPTION TO THE
EXTERNAL SURFACE

OF THE HUMAN BODY.

By JOSEPH KLAPP,

OF ALBANY, NEW-YORK,

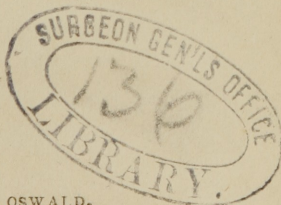
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“Fiat Experimentum.”

PHILADELPHIA:

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.....
1805.



CHEMICO-PHYSIOLOGICAL
INAUGURAL ESSAY,
ESSAY,

RELATIVE TO THE DEGREE OF INFLUENCE OF THE

DOCTOR OF MEDICINE.

TO THE EXAMINATION

OF THE CANDIDATE

REV. JOHN ANDREW D. D. PROVOST, (Pro Temp.)

BY JOSEPH KLAPP,

TRUSTEES AND MEDICAL FACULTY

UNIVERSITY OF PENNSYLVANIA

THE 20th DAY OF JUNE, 1850.

PHILADELPHIA.

PRINTED FOR THE AUTHOR, BY J. B. LIPPINCOTT & CO.

1850.

AN
INAUGURAL ESSAY,
FOR
THE DEGREE
OF
DOCTOR OF MEDICINE,
SUBMITTED
TO THE EXAMINATION
OF THE
REV. JOHN ANDREWS, D. D. PROVOST, (Pro Tem.)
THE
TRUSTEES AND MEDICAL FACULTY
OF THE
UNIVERSITY OF PENNSYLVANIA,
ON
THE 3d DAY OF JUNE, 1805.

For Doct: Cleaver.

With the esteem of his sincere,

friend & fellow Graduate.

The Author.

TO
JAMES WOODHOUSE, M. D.
PROFESSOR OF CHEMISTRY,
IN THE
UNIVERSITY OF PENNSYLVANIA.

SIR,

TO you this imperfect Essay is dedicated, not with the vain expectation of giving additional fame to a reputation already established, but solely for the purpose of expressing my thanks for the friendly attention which you have bestowed upon me while in this city; and the high sentiments which I shall ever cherish, for your unrivalled talents as an able instructor and scientific Chemist.

THE AUTHOR.

JAMES HUTCHINSON, M. D.

THIS ESSAY

IS ALSO DEDICATED

AS A MARK OF RESPECT

FOR

HIS PROFESSIONAL TALENTS,

AND ESTEEM FOR HIM,

AS A WORTHY FRIEND AND COMPANION.

BY THE AUTHOR.

THE AUTHOR.

TO
PHILIP S. VAN RENSSELAER, Esq.
OF ALBANY, NEW-YORK.

DEAR SIR,

TO a heart glowing with lively impressions of gratitude, an opportunity of acknowledging, in this public manner, the favors conferred by the kind hand of friendship, must afford sensations of the purest kind that throb in the human bosom.

BROUGHT UP and educated under your immediate care and direction, I am conscious that nothing has been neglected which would promote my future establishment and welfare in life : even for the great misfortune of being prematurely deprived of an affectionate Father, I have, in your indulgent guardianship, found an alleviation, which none but you could give.

WITH this small though genuine tribute of veneration and esteem for those engaging qualities which have long distinguished you, whether beheld in domestic or in public character, as the accomplished Gentleman, and the zealous admirer of virtue and science, permit me to offer up an earnest prayer for the prolongation of that life, which has already rendered much good to society ; and for the parental guidance which has been so properly exercised towards me, accept the thanks of your

SINCERE FRIEND,
AND HUMBLE SERVANT,
THE AUTHOR.

To Mr. JOSEPH KLAPP.

Dear Sir,

IT AFFORDED me great satisfaction to witness the zeal and industry with which you conducted the enquiry which has led to the important deductions contained in your Inaugural Dissertation, disproving the existence of an æriform function in the Skin.

WITH pleasure I bear testimony to the neatness and accuracy with which your experiments were performed, and consider them as completely satisfactory on this interesting subject.

THAT the light which you have thrown on this question may stimulate you to extend your enquiries to other branches of the science of Medicine, and that you may enjoy health and happiness in life, and meet with success in the exercise of your profession, is the ardent wish of

Dear Sir,

Your sincere & affectionate friend,

JAMES WOODHOUSE.

May 3d, 1805.

INTRODUCTION.

A favorite author, not less distinguished for literary accomplishments, than correctness of observation, says, a composition that enters the world with a view of improving it, has a claim to the utmost indulgence, though it fail of the effect intended. If such encouraging lenity await the voluntary writer, whose object to instruct is often allowed by an indignant critic for public adulation, my claims on the score of indulgence cannot but meet with liberal success, as nothing less urgent than a law of the University could have induced me, at this juvenile period of my life, to appear in the character of an author.

The subject which has been selected for this essay, is one which has been much canvassed by men of acknowledged talents; many of the difficulties, however, attending its satisfactory investigation, were either left neglected, or imperfectly surmounted, though it for a while attracted the experimental attention of a Crankshaw, a Priestley, and an Abernethy.

The present enlightened state of medical science excepts none from the privilege of laboring in her enchanting dominion; and as invaluable truth is alike appreciated, whether detected by the searching penetration of irresistible genius, or discovered by the good fortune of a mere novice in medicine, I have been induced to venture on the subject under consideration, without pausing to offer an apology for interfering with the labors and opinions of such eminent characters.

.....and truth alone,
Shall be our chosen theme, our glory to the last.
(OVER.)

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COWPER.

The theoretical and practical advantages derived from a correct knowledge of the laws and operations of the animal body, are too generally valued, to require commendation, and the pernicious tendency of ascribing functions to parts not recognised by the æconomy of nature must be evident to every reader.

When we recollect that just ideas of the nature and cure of disease, cannot be obtained without the aid of physiological instruction, the necessity of cultivating this important branch of our science with nicety and carefulness will occur to the most of observants, and the often unfortunate consequences of mistaken publications cannot be exemplified in a more striking manner than a candid disclosure of the error in which I was nearly involved.

The transpiration and absorption of the gases by the skin, as apparently demonstrated by authors of unquestionable integrity and reputation in medicine, have for some time past engaged my consideration, and so firmly did I believe in their proofs and observations, that a series of experiments were actually instituted to ascertain the influence and connection of these functions with the pulmonary forms of fever. But the result of a few of their most interesting experiments carefully repeated, gave me reasons to believe that there was some fallacy in the case, and a further enquiry, which was then anxiously carried on, soon confirmed my suspicions.

The experiments which constitute the basis of this essay were conducted without any preconceived notions which could bias their natural import, and the fidelity with which they are detailed, can be attested by a few ingenious friends who had the goodness to assist me in their performance: but how far they will go to effect a decision on the subject, I leave to be determined, with all due deference, to the better judgment of those who may hereafter give them an attentive examination.

AN

INAUGURAL ESSAY,

Disproving the existence of an æriform function in the Skin, &c.

PART I.

PHYSIOLOGISTS have long laboured with diligence to discover the functions of the Skin.

The insensible emanations from its surface were known to Galen; and the great Sanctorius proved, in his statical experiments, that they consisted of watery vapour, endowed with certain excrementitious qualities.

Since the days, however, of these two celebrated Physicians, new researches have been made, and many of which have ended in imputing to this organ a more important office in the animal body.

Count De Milly recorded a paper in the memoirs of the Royal Academy of Sciences, at Berlin, in the year 1777, in which he adduces experiments in proof of the discovery of an elastic fluid which passes off by the Skin. The imperfect manner in which experimental enquiries were conducted at this period of physiological improvements, necessarily connected with chemical knowledge, gives many reasons for believing that the Count was mistaken; particularly, when it appears, from the experiments of a judicious and more modern writer,* that no

* Abernethy.

correct inference can be drawn, of the æriform function of the Skin from the analysis of gases collected, unless they were first received over quicksilver. Count De Milly says, being in a warm bath, half a pint of air might have been collected over the water, in the space of three hours ; and from his analysis, which was both inaccurate and very incomplete, says an author * in Nicholson's Journal, he concluded it was carbonic acid gas.

Two years subsequent to the publication of the above memoir, Mr. Cruikshank, an ingenious Physiologist of England, gave to the world an essay on insensible perspiration. His object being chiefly to investigate the quality of the watery vapour emitted by the Skin, much information on the æriform discharge could not be expected.

The following experiment, however, I will take the liberty of detailing, as the only one, in his publication, that relates to our subject. He introduced his foot, previously washed and dried, into an empty bottle, excluding the access of external air by means of a moistened bladder ; after retaining it in this situation for one hour, he withdrew it, " the fluid collected " says he, " produced no change on the lime water, but lime water thrown into the bottle and agitated became as turbid as when the air in which the wax taper had extinguished itself, was mixed with it. " This last experiment " continues Mr. Cruikshank " I repeated several times and with the same success ; from this I inferred, that admitting the common theory of fixed air and Phlogiston, something passed off with the vapour of insensible perspiration of the skin which rendered air fixed." Inductions from incorrect data have always been fruitful sources of error in medical philosophy. This experiment, for two good reasons, cannot authorise the conclusion which Mr. Cruikshank has deduced ; first, because a moistened bladder will, by furnishing carbon to the oxygenous portion of the air in the bottle, yield fixed

* *Trousset.*

air ; and secondly, because lime water poured out of one bottle into another, with the addition of agitation, will become turbid, from the chemical union of the carbonic acid of the atmosphere with the calcareous earth, as any one will find by making the experiment.

Doctor Ingenhouz, about this time, paid some attention to the nature of the bubbles of air which he saw arise from the skin while immersed in water ; but his experiments are few, and are liable to many objections that apply to De Milly's and others who have not taken the necessary precaution of ascertaining the nature of the gases contained in the water in which their experiments were made.

Mr. Troussset, a French Physiologist, has published an account of his experiments made in water, in which the transpiration of carbonic acid gas is denied, but the emission of azotic air by the skin is contended for.

That gases are disengaged from the surface of the body, while immersed in common water, no one will pretend to deny ; but that they are prepared in the body, and emitted by minute vessels which open on the skin, is fully refuted by all the experiments detailed in the sequel of this Essay. If then, the air collected, does not come from the vessels of the skin, we must look to the water as the only source which can, under these circumstances, yield it.

As plants, under the influence of solar light, will readily disengage from pump-water, and the most of other waters in which they are placed, a considerable quantity of pure air, and as Professor Woodhouse has clearly proved, from a number of decisive experiments, that it is produced by the decomposition of carbonic acid with which the water is impregnated, I may conclude that the generality of waters contain fixed air ; and that waters are more or less impregnated with azotic air, according to local circumstances, is now too well ascertained, to

be disputed by any one. With these considerations, founded upon incontrovertible truth, I do not hesitate to determine, in my own mind, that the evolved heat of the hand, foot, or of the whole body, which were the subjects of experiments under the direction of different Physiologists, expanded the gases contained in the water which they used, and thus caused them to rise in bubbles of air from the surface of the skin. This conclusion is rendered more probable, when we recollect the experiments which have long since been made by Dr. Priestley in water exhausted of all its air, in which not a single bubble of any gas whatever was seen to arise, though the skin was immersed in it for a considerable time.

The Doctor expressly declares, that the perspiration of animals does not contaminate common air as the process of respiration does. I am well aware it is asserted that water absorbs carbonic acid as fast as it is emitted by the pores of the skin, and that this circumstance accounts for the result of the experiments alluded to. But to this I ask leave to reply, in the first place, that fixed air does not so rapidly unite with water as to preclude an opportunity of detecting it, if any were thrown out; and in the second place, that this objection does not apply to this mode of experimenting to discover whether azotic air is ever thrown out in perspiration, as many Physiologists assert; and as they have erred in one instance, it is more than probable, from what has already been advanced, that neither carbonic acid nor azotic air, is ever thrown out in the perspiration of animals. But what renders the explanation which I have given, of the origin of the gases collected over water, in which the hand or the whole body were immersed, still more satisfactory, is the simple, though not less conclusive experiment which was made, by holding my hand and forearm, [previously invested by varnished silk which was impervious to air] in a tub of pump water: in a short time innumerable sphericles of air were seen

both by my friend and myself, to form on the external surface of the silk, and gradually rise to the top of the water.

The successive formation and disengagement of minute bubbles of air, being always confined to the surface of the skin, while immersed in water, and not proceeding from portions of water remote from its surface, may be urged as an argument in favor of gaseous transpiration ; but error, from this quarter, would be too palpable, to elude detection ; vegetables, while immersed in water impregnated with carbonic acid, and exposed to the light of the sun, disengage bubbles of pure air from the surface of their leaves ; yet who will contend that it comes from their fibrous texture ? In my humble opinion, the disengaged heat of the skin enables us to explain, as clearly, the one fact, as the decomposition of fixed air does that of the other.

The embarrassment in which many Physiologists have involved themselves, on the interesting subject of the functions of the skin, I regard as similar, in many of its concomitants, to the once prevalent, though erroneous opinion, that vegetables purify atmospheric air, by absorbing its azotic, and perspiring its oxygenous portions. This opinion, however, by the light of modern discoveries, has been exploded, as an unhappy hypothesis, originating out of incomplete researches ; and for an explanation of the source of the air disengaged by healthy plants, under water, we are very justly referred to the previous condition of this fluid.

Mr. Abernethy, a practical physiologist, and surgeon, of London, published in the year 1793, an essay on the functions of the skin, in which he maintains, by a variety of experiments, that not only fixed air and azotic gas are thrown out by the skin ; but that oxygen air and carbonic acid gas are very readily imbibed ; whilst the nitrous, hydrogenous and nitrogenous gases tardily gain admittance into the absorbing vessels. I do not

think it necessary to detail, in particular, the experiments which Mr. Abernethy performed, but will mention that the chief of them were made by holding his hand and wrist in an inverted glass vessel, and in a medium of different gases confined over mercury. He also made two or three experiments in water, but in these he could not discover that carbonic acid gas was emitted by the skin; believing that the water absorbed the gas, he soon discontinued this mode of prosecuting his subject.

M. Seguin, in 1792, delivered a paper on the functions of the skin, to the Royal Academy of Sciences, at Paris. The accuracy and precision with which this philosopher conducted his experiments, have never been questioned; yet it is well known, that they afford a result entirely different from those of Mr. Abernethy. M. Seguin, satisfactorily proved, in his paper, by a great number of ingenious experiments, that the skin, with its cuticle entire, absorbed neither air nor water.

Mr. Fourcroy declares, that "it is not true that there pass off, through the skin, as some moderns have asserted, elastic fluids, and especially carbonic acid gas."

After having adduced the above respectable authority in behalf of the sentiments which are laid down in this essay, I will first venture a few observations, founded on anatomical and physiological knowledge, pointing out the improbability of an æriform function in the skin, and then proceed to detail the experiments which fully authorise my disbelief in such an operation. In the very commencement of this undertaking, I am met by a conviction of being incapable of rendering that justice to truth, which is requisite for its permanent establishment in the records of medical science; but, as the maxim of the late illustrious and unfortunate Lavoisier, is deeply engraven upon my mind, I feel but little apprehension of being deluded from the proper track, by the fascinating incentives of hypothesis.

“ We ought, in every instance, to submit our reasoning to the test of experiment, and never to search for truth, but by the natural road of experiment and observation.” *Lavoisier*.

When assertions are made, either of a practical or theoretical nature, from a set of experiments, or a train of plausible reasoning, if, upon a close examination, they are found not to accord with the experiments of others, well conducted, and more coincident with the economy and operations of the animal body, it has long been a standing rule in medical philosophy, to consign them to merited oblivion.

On physiological subjects, correct anatomy will always be regarded as the polar star to successful researches ; and when she has performed her duty, the established laws of the animal body, like the compass which guides the ship at sea, ought only to be entrusted with the important office of conducting us to the end of our journey.

Mr. Abernethy says, “ the removal of a quantity of oxygenous gas, from common air, is surely a curious circumstance ; if this be the effect of an action in the absorbing vessels, it must much exalt our ideas of their subtilty, and their aptitude or disposition to admit one species of matter, and to reject another. That the absorption of one air, in preference to another, depends upon this cause, I believe will not, upon reflection, be doubted. It might indeed be suspected, that oxygenous gas was separated from the atmosphere, by the skin, as it is in the lungs, by chemical attraction : but it has been proved, that carbonic acid is removed with equal celerity ; and experiments on animal substances shew, in them, a disposition rather to part with, than to imbibe, carbonic acid. The removal of this air is, therefore, not likely to be the effect of chemical affinity.”

My

My objections to this reasoning of Mr. Abernethy are, first, his data are far from being established ; on the contrary, the repetition of his chief experiments in my hands, fully disprove the existence of a gaseous function in the skin. The highly ingenious researches of Seguin, before mentioned, have given conclusions directly opposed to those of Mr. Abernethy ; but they correspond with the result of my enquiries. In the second place, lymphatick absorbents cannot be satisfactorily demonstrated on the skin, though the best dissections have not been wanting, to ascertain their presence. The science of Physiology only embraces the healthy actions and powers of parts, the existence of which have been previously known to the anatomist ; all beyond these prescribed limits must be uncertain, if not entirely conjectural. But if oxygenous, carbonic and other gases are taken up by the skin, and carried into the system, why have they not been detected, either in their gaseous form, or incorporated with the fluid circulating in the absorbing vessels ?

The next question that naturally occurs, is, how are these gases disposed of, admitting for a moment that they are actually taken in by the lymphatick vessels of the skin ? As Mr. Abernethy has excluded chemical attraction from any agency in the case, they must be conducted through the course of the absorbents to the subclavian veins. The quantity of oxygen gas absorbed from the surface of the whole body, must be considerable, if the absorbents of the hand and wrist are capable of imbibing eight ounces in eight hours, as Mr. Abernethy asserts ; would not then the blood in the left subclavian vein, which receives the thoracic duct, be of a more florid colour than that of the right, which only receives the lymphaticks of the right arm, and of the right side of the neck ? And lastly, if oxygen gas be absorbed by the skin, and conducted through the course of the lymphatick vessels into the general circulation, may we not call in question the œconomy of the animal body

as the lungs appear fully competent to all the purposes of oxygenating the system.

A Chemist of this city, whose judgment I have reason to place great confidence in, informed me, that if gases were given out by the skin, they might be discovered by holding the arm in a glass vessel filled and inverted in water. As I had ascertained the nature and proportions of the gases contained in the pump water at the Pennsylvania Hospital, where some of my experiments were made, no doubt was entertained but that some information might be derived from this mode of experimenting. Accordingly, a few experiments were made in this manner; as water, however, has been objected to, I will not relate them in particular, but deem it only necessary to mention that the gases collected, differed in no considerable degree, from those which had been previously obtained from the water.

As mercury has never been objected to, as a proper medium to experiment in, for accomplishing the objects of our present enquiry, a sufficient quantity was procured, and a tub arranged with necessary conveniences to contain it, as I was fully persuaded, that if any airs were thrown out by the skin, they could, under the circumstances of the following experiments, be easily detected.

EXPERIMENT 1. *Thermometer 73°*

A glass vessel being filled with, and inverted in quicksilver, all air that adhered to my hand and wrist was carefully separated by moving them in different directions, under the surface of the quicksilver for ten minutes; they were then introduced into the inverted vessel and there retained for more than two hours; for a few minutes, in the beginning of the experiment, my hand was in some degree benumbed with cold, but its usual warmth soon returned, and at the expiration of the time mentioned, little or no inconvenience was experienced, but

what arose from the pressure of the mercury. During all this time, not a bubble of either carbonic acid or azotic air, was seen to emanate from the skin. This experiment was made eight months ago, and has since been repeated in the presence of Professor Woodhouse, with precisely the same result.

As lime water has always been regarded as a correct test for carbonic acid, it appeared plain, that if fixed air was thrown out by the skin, it would, while the hand and wrist were immersed in it, form, by chemical combination, carbonat of lime, and the milky color produced would afford a good criterion to judge of its emission. Under this impression the following experiment was made.

EXPERIMENT 2. *Thermometer 70°*

My hand and wrist were introduced into a jar containing lime water, whose purity was previously ascertained, and held in this situation for one hour ; during this time no carbonat of lime was formed, but on the contrary, the lime water, on withdrawing my hand, was as transparent as it was previously to its use. This experiment was repeated at another time, with no other variation than that of its being continued two hours, the result of which was the same as in the first ; so that no deception could have occurred in its performance.

Mr. Abernethy, firmly believing that elastic gases were emitted by the skin, objected to water as a medium to experiment in, to discover this function, on account of its great disposition to absorb carbonic acid ; all experiments, therefore, made in this manner, must be incomplete as relating to this acid gas, unless the state of the water be attended to, subsequent to their performance. With this consideration the following experiment was instituted.

EXPERIMENT 3. *Thermometer 58°*

Having half filled a convenient vessel with water, that had been previously boiled for some time, in order to separate all

loose air that it might be impregnated with ; my foot and ankle were immersed in it, and retained in this situation for three hours, and then withdrawn. My object was now to ascertain, whether or not the water had imbibed carbonic acid ; to accomplish which, a glass vessel was filled and inverted in the same water, a small handful of healthy leaves, of a species of geranium, were introduced into the inverted vessel, and the whole exposed to the rays of the sun for four hours, during which time, no oxygen air was disengaged from the water. In this experiment, we may with safety conclude, that carbonic acid was not contained in the water, and consequently none was emitted by the skin ; for it has been proved to a certainty, that healthy leaves of plants will readily decompose carbonic acid, if contained in the water in which they are immersed, while in the light of the sun ; its coal will be devoured for food, and its oxygen escape in the form of pure air.

Lest a few of those who may honor this dissertation with a perusal, should hesitate in admitting experiment 1, as satisfactory, on account of the pressure of the quicksilver, the following one was made, which proves, beyond a doubt, that carbonic acid gas is not emitted in the perspiration of animals.

EXPERIMENT 4. *Thermometer 56°.*

Having procured some pure hydrogen air, from diluted sulphuric acid, and the filings of malleable iron, four ounce measures of it, were thrown up into a glass vessel, previously filled and inverted in quicksilver : my hand and wrist, after all loose air was separated from their surface, as in experiment 1, were introduced into the inverted vessel, and kept in this situation, for three hours. My hand and wrist, being surrounded by the hydrogen gas, in this experiment, suffered nothing from either the pressure or coldness of the quicksilver. Professor Woodhouse observed this, as well as the chief experiments detailed in this essay ; and in his presence, the air in the vessel from which my hand and wrist had just been removed, was examin-

ed. Having ascertained that the volume of air had not been, in any degree, diminished, we passed up one ounce measure of it, over lime water, in an eudiometer; no milky appearance was observed by either of us: about three ounce measures of lime water were then passed up into the vessel containing the remainder of the air in which the experiment was conducted; but no carbonic acid could be discerned, in either case.

Dr. Rush relates, in his lectures, an experiment which induces him to believe in an æriform function of the skin: a lighted candle, he says, in the morning, was extinguished by the air of the bed in which he had slept, the preceding evening. I do not hesitate to admit the experiment, as correct, but cannot subscribe to the inference of our ingenious Professor.

The readiness with which respiration renders air impure, from the consumption of its oxygenous portion, is well known to every physiologist, and in my humble opinion, the contamination of the air in the bed, was owing to this process. This explanation will appear more probable, when we consider how often we lay, in the course of an evening's sleep, with our heads under the bed clothes.

Having discovered, by all the above experiments, that the skin, with its membranes entire, has no connection with the office of transpiring elastic fluids, I will next relate those experiments, which, in my own mind, undeniably prove, that gases are not absorbed from the external surface of the human body.

EXPERIMENT 5. *Thermometer 60°*

Five ounce measures of atmospheric air were thrown up into a glass vessel, previously filled and inverted in quicksilver; my hand and wrist, with the necessary precautions used in experiments 1, and 4, were introduced into it, and retained in this situation, with little or no inconvenience from the mercury, for three hours, and then withdrawn. We immediately exam-

ined the air in the vessel ; its volume was not diminished, and, when passed through lime water, no milky color ensued ; which was an additional proof that the skin did not throw out carbonic acid ; we next proceeded to ascertain whether or not any of the oxygenous portion of the air had been removed during the experiment ; for this purpose, one measure of it was passed up, over water, in an eudiometer charged with phosphorus, and in less than twenty four hours, the absorption of $\frac{22}{100}$ was complete. In this experiment, no addition or alteration occurred in the atmospheric air to which my hand and wrist had been exposed for three hours ; but with the view of giving complete satisfaction on the subject, the following experiment was performed.

EXPERIMENT 6. *Thermometer 54°*

Five ounce measures of oxygen air, of 4 per cent purity, being transmitted into an inverted vessel, previously filled with quicksilver, my hand and wrist, freed from all adhering air under the surface of the mercury, were introduced into it, and held in this situation three hours. Upon examination, the quantity of air had suffered no diminution ; it was then passed through lime water, and as turbidness did not occur, we inferred that carbonic acid had not been given out by the skin ; the air was next tested, as in the former experiment, in the eudiometer of Berthollet, and was found not in the least adulterated.

The experiments just related, prove, in the first place, that carbonic acid is not transpired ; in the second place, that azotic air is neither emitted nor absorbed ; and in the third place, that oxygen air is not inhaled by the skin of the human body. The care with which they were conducted, and the completeness of their plan, exclude all the deception which experimental enquiries, in the hands of inattentive observers, are often liable to.

In order to discover whether, or not, fixed air be removed from the external surface, by cutaneous obsorbents, the following experiment was made.

EXPERIMENT 7. *Thermometer 61°*

Five ounce measures of carbonic acid, obtained from carbonat of lime, by diluted sulphuric acid, were transmitted into a glass vessel, previously filled and inverted in quicksilver; my hand and wrist were then introduced into it and kept in this situation for three hours. The volume of air was next attended to, in order to accomplish the object of the experiment; it was found diminished in quantity only about half a drachm. The removal of which, I do not hesitate to ascribe to the combination of the carbonic acid with the perspirable fluid; as my hand, when removed from the inverted vessel, was considerably moistened by this discharge. When we call to mind the affinity that naturally subsists between aqueous substances and this acid gas, the effect which we have mentioned is what ought to be expected; besides, it is highly improbable that the skin, even if it were endowed with the power of absorption, would inhale carbonic acid which is thrown out by the lungs, as excrementitious, from the system, and refuse to absorb oxygen air, confessedly the pabulum vitæ, as was the case in experiments 5th and 6th.

The experiments are now related, which have created, in my own mind, a firm belief, that a few physiologists, for whose talents in the science of medicine, I shall still cherish the highest sentiments of respect, have, from the illusion of unforeseen circumstances, which often render of no avail the best directed enquiries, erroneously imputed an æriform function to the skin.

It is to Mr. Abernethy, whose pre-eminent abilities and industry have long entitled him to the first rank in his profession, that we are indebted for the best mode heretofore used in

the investigation of our subject. But while I am recording the merit which so justly belongs to this distinguished character, perhaps the zealous friends, and cultivators of Physiology will regret, that his ingenious researches had not terminated in the acquisition of truth. Their motives on this interesting occasion would, I am conscious, have sufficient grounds of support; for one great source of ignorance and uncertainty in medicine is the accumulation of experiments and plausible theories, which are directly opposed to each other.

In taking leave of this part of my essay, it will not be amiss to observe, that the preceding, as well as the following pages, are respectfully submitted to the examination of the public, not with the expectation of dislodging, immediately, opinions heretofore credited, but solely with the hope of calling the attention of others, who may ingenuously decide on the subject of the controversy; as I shall be satisfied, either in being supported or refuted, by such an interference.

EXPERIMENTS,

AND

OBSERVATIONS ON THE IMPROPRIETY

OF ASCRIBING ABSORPTION TO

The External surface of the Human Body.

PART SECOND.

WHEN first the duty devolved on me to write an Inaugural dissertation, I intended to have confined myself to the consideration of those subjects which are above treated of; but as time was not wanting, and the usual limits allotted to a publication of this kind, were not exceeded, I resolved to extend my enquiries still further into the functions of the skin.

Physicians, both in ancient and in modern times, have been at a loss to account for the manner in which many medicines produce their respective effects upon remote parts of the animal body, when only applied to the external surface.

Some have supposed that this phenomenon in medicine, could not be satisfactorily explained, without admitting, that a portion of the articles were taken up by the absorbing vessels of the skin, and conveyed into the general circulation, to be directed to their appropriate parts, and there give rise to effects corresponding with the nature of the medicines employed.

In behalf of this theory, we have on record the works of many respectable physicians, among which none are so eminently entitled to our consideration as the experiments and

observations of Dr. Alexander Monro.* The doctor made a great variety of experiments on frogs, in different conditions, all of which tend to prove that, in them, opium, ardent spirits, and essential oils are absorbed from the external surface. He found that after the crural nerves of one frog were cut, and the hind half of the spinal marrow of two others completely destroyed ; camphor applied to the hind legs of all, produced the same effects, in nearly the same time, as when applied to a sound animal. "But to prove this absorption in fact," says our learned author, "I divided two frogs at the pelvis, two hours after the camphor had been applied to them in the above way ; I then pulled the skin off the fore part of their body ; and found, that the flesh and bowels had a smell of the camphor. To discover this more certainly, I cut them in pieces, and poured on one, rectified spirit of wine, and on the other, water ; and was sensible of the taste of the camphor, both in the spirit of wine, and in the water." As all the experiments made by the Edinburgh professor, were conducted without any regard to the lungs, which I shall soon prove to be an extensive apparatus of absorption, I do not hesitate to pronounce them incomplete, as the articles which he used might have passed into the system through this quarter, and not absorbed from the external surface, as he inferred. But even if I was disposed to allow that his experiments completely prove that absorption does take place from the surfaces of frogs, they cannot, with any certainty be used in the investigation of our subject. An amphibious animal is widely different, in its anatomical structure, way of life and its æconomy, from man. If then, it should hereafter be satisfactorily ascertained, by a series of new experiments, or by a repetition of those of the professor, with a proper variation, that the frog is endowed with the function of cutaneous absorption, the discovery would not, in my mind, excite surprise, even if I should establish, by the experiments shortly to be detailed, that this office is unconnected with the æconomy of the human body.

Dr. Barton, who perhaps is better acquainted with the functions of the lymphatick system, than any other man in America, is inclined to give some credit to cutaneous absorption in frogs. He informed me, in private conversation, that he had frequently observed, that if this animal was confined in a dry glass vessel, it became enfeebled, diminished in its natural size, and scarcely able to leap; but if a small quantity of water was poured into the vessel, or the air in it only loaded with moisture, it soon acquired its wonted vigour, its body became plump, and its motions were usually lively. In the first part of the sixth volume of the Philosophical Transactions, an account is given of the *Lacerta Subviolacea*,* in which it is mentioned, that this animal was weighed at different times. On the 24th of March it weighed 342 grains, but in somewhat less than an hour, it weighed only 324 grains, having lost 18 grains. "It is a well ascertained fact, however," says the ingenious naturalist, Dr. Barton, "that the weight of many of the Amphibia, particularly the frogs and lizards, is very various at different times, even in the course of the same day or hour. This difference of weight is often entirely independent on any aliment, whether solid or fluid, being taken into the stomach, and must be ascribed to the absorption of water."

Dr. Darwin, the ingenious author of the *Zoonomia*, says, "that those who have remained half an hour in a warm bath, when they have previously been exhausted by exercise, or abstinence from food or fluids, have absorbed so much as to increase their weight considerably. Dr. Jurin found an increase of weight to 18 ounces, by sleeping in a cool room, after a day's exercise and abstinence; so much, in that situation, was absorbed from the atmosphere."†

* *A species of Lizard first described by Dr. Barton, and called by him Lacerta Subviolacea.*

† *Zoonomia, page 440.*

After having read the fair and conclusive experiments of Dr. Currie, the assertion of Dr. Darwin will appear highly erroneous ; for in one instance, where death was caused by inanition, no cutaneous absorption occurred, though the patient was immersed in a warm bath for an hour. In Dr. Jurin's experiment, the absorption doubtless took place from the lungs, and not from the external surface.

It is argued in favor of absorption, that animals live in hot, moist climates, without drink, and yet discharge a considerable quantity of humors, both by perspiration and urine ; and that in some diseases of the human body, a much greater quantity of urine is discharged, than the quantity of drink taken in. That animals do live in hot, moist climates, without drink, we will readily admit, but that their thirst is allayed, their cutaneous and urinary discharges carried on, in consequence of absorption by the skin, is an induction which modern discoveries will not countenance ; for doubtless, in them, as in cases of hydropic and other diseases, where a much greater quantity of urine is often discharged than the quantity of drink taken in, the lungs supply the exigencies of the system. And what more particularly renders my reasoning at least plausible, on this subject, is the experience of a number of respectable physicians.

Dr. Rollo* published an account of an interesting case of diabetes, in which he mentions that the weight of the patient was not increased by a continuance of ten minutes, in a bath of 110 degrees of heat. " In the year 1788, Dr. Currie happening to be at Buxton, made an experiment on the effects of bathing, on the weight of the body. After half an hour's immersion in the bath, he found his weight rather diminished than increased. In the year 1790, he had a patient in diabetes, whose cuticle, as is usual in that disease, was in a morbid state. Being de-

* *Zoonomia*, page 255.

sirous of trying how far the inordinate action of the kidneys might be affected by a gentle stimulus applied to the skin, he immersed him in a bath of the temperature of 96 degrees, weighing him before and after the immersion. In this case, no variation in the weight could be detected. Dr. Currie afterwards made five different experiments of the same kind, upon himself, varying the heat of the bath, from 87 to 95 degrees; but he could never, in any one instance, find his weight augmented. It may however be said, that though in diabetes, where the epidermis is diseased, no liquid is inhaled; and tho' in health, when the vessels are full, no absorption may take place; yet, when the body is wasted, from a want of proper food through the stomach, the plastic powers of nature may be employed to supply the defect, and to excite an inhalation through those pores on the surface, by which an exhalation is usually performed. To prove that this does not happen, Dr. Currie relates, very minutely, a remarkable case of dysphagia, where death was the consequence of inanition, notwithstanding every attempt to support the system, both by the rectum, and by the surface. This patient, on different occasions, stepped perfectly naked, upon Merlin's balance, immediately before immersion; and again immediately after it, his body being previously dried. The weights were never moved. The result was surprising; for Dr. Currie could not distinguish the slightest variation in the weight of the body, though the beam would have detected a single drachm, though the immersion had been continued for an hour, and though a constant friction had been kept up, nearly the whole time, on the inner surface of the thighs, with the view of encreasing the action of the absorbents. If the non-absorption by the surface of the body be established, it will, Dr. Currie observes, ascertain, that, in the ordinary course of things, contagion is received into the system, by the lungs only, and will, he thinks, justify a practice, which he has been informed, is common among our more experienced seamen, on the coast of Guinea, and other warm climates, who when exposed, during the night, to a breeze from the marshes

wrap their heads in a sea-cloak, or other covering, and sleep fearless on the deck, with the rest of their bodies nearly exposed."

Since the celebrated French Philosopher, Bichat, suggested that absorption took place from the lungs, and published the experiments corroborative of his opinion, we have not been at a loss to account for the manner in which certain volatile articles applied to the skin, have, in a short time, passed into the system, and manifested their presence by the effects which they produce on the excretions of the body. The following quotation from this author, will afford the reader an epitome of his theory.

Extract from a work entitled "*Recherches Physiologiques sur la Vie et la Mort* : par XAV. BICHAT, Professeur d'Anatomie et de Physiologie. A Paris, an 8." Page 352. "La respiration d'un air chargé des exhalaisons qui s'élèvent de l'huile de thérébentine, donne aux urines une odeur particulière. C'est ainsi que le séjour dans une chambre nouvellement vernissée influe d'une manière si remarquable sur ce fluide. Dans ce cas, c'est bien évidemment par le poulmon, au moins en partie, que le principe odorant passe dans le sang, pour se porter de là sur le rein ; en effet, je me suis plusieurs fois assuré, qu'en respirant dans un grand bocal, et au moyen d'un tube, l'air chargé de ce principe qui ne sauroit alors agir sur la surface cutanée ; l'odeur de l'urine est toujours notablement changée." "The respiration of an atmosphere charged with the vapours of spirit of turpentine, gives the urine a peculiar odour. Thus, after remaining some time in a chamber newly painted, we find that a remarkable change is produced in this fluid. In this case, it is extremely evident, that, through the lungs, at least in part, the odorous principle passes into the blood, and from thence to the kidneys. In fact, I have frequently convinced myself of it, by breathing an air charged with this principle, through a tube adapted to

“ a large glass vessel, (by which means the cutaneous surface
 “ of the body could not be acted on) and always found the o-
 “ dour of the urine remarkably changed.”

In this experiment, as the vapour was not applied to the surface, it could only act upon the lungs.

The experiments and observations of the Parisian Professor, are fully established in an inaugural essay by Doctor Rosseau, an ingenious French physician, of this city.*

As many articles, however, occasionally applied to the skin, for medicinal, and other purposes, are not endowed with an odour sufficiently volatile to gain access to the lungs, so as to effect any remarkable change in the system, the operation which they produce, must, therefore, be referred to the reciprocal harmony which subsists between the various parts in the organized body. Tartrite of antimony, rubbed upon the skin, excites nausea and vomiting; which are the usual symptoms produced when it is taken into the stomach.†

A poultice of tobacco leaves applied to the region of the stomach, excites an emetic operation, after large doses of sulphat of zinc have been given without producing this effect.‡

Opium produces sleep, when externally applied; and in certain affections of the stomach, many physicians, and particularly Dr. Whytt, have availed themselves of the knowledge of this circumstance with manifest advantage to their patients.

*Dr. Rosseau made his experiments in this city, about the same time that Professor Bichat published his physiological work, in Paris.

† Dr. Barton.

‡ Sherwin's experiments have since been repeated by a Graduate of this University, and found correct.

Dr. Barton informed me that he had cured several cases of intermittent fever, by applying poultices of the Cinchona bark, to the skin. These, and other active articles, in my humble opinion, produce their respective effects on the animal body, through the medium of sympathy; as the theory which supposes their absorption, is involved in too many difficulties to be generally received.

I am well aware that professor Monro has made experiments which may induce some to believe that opium acts chiefly upon the system, after being applied to the skin, in consequence of its absorption. But I have before observed, that the results of experiments made upon frogs, cannot, with any propriety be contrasted with those of experiments made on the human body.

Having now premised the facts and observations which are necessarily connected with the investigation of my subject, I will next proceed to lay before the reader the experiments which demonstrate, in the first place, that the lungs furnish an extensive surface for absorption; and in the second place, that the skin of the human body has no claim to that function.

The important experiments of Bichat, and those made in this city by Dr. Rosseau, have certainly given an extensive range to our views of the Physiology of the animal body; but as the emanations from spirits of turpentine which they breathed, might have been conveyed either in part, or wholly, into the system, by the absorbing vessels of the mouth, fauces and trachea, over which parts this active article passed before it could have come in contact with the lungs, their experiments, in my opinion, do but incompletely prove what they have adduced.

With the intention, however, of ascertaining this subject more certainly, I made the two following experiments; for

assistance in their performance, I am indebted to my worthy friends and fellow graduates, Messrs. Smith and Legaré.

EXPERIMENT I.

A dog was secured upon a table, by passing leather straps, in different directions, around his body and extremities. An incision was made through the skin and muscles, just above the upper end of the sternum ; and the trachea was laid bare ; a strong ligature was passed around it so as to intercept all communication between the lungs and the mouth of the animal. A longitudinal opening was then made, immediately below the ligature, into the trachea, to which was adapted one end of a long tube, the other end being passed out of a window. In this situation the dog breathed the air of the atmosphere, through the tube, for more than two hours, during which time his mouth, fauces and trachea above the ligature, were frequently inflated with the vapours of spirit of turpentine. Now it is evident, that if absorption exclusively belongs to the lungs, under the circumstances of this experiment, the vapours of spirit of turpentine cannot be conveyed into the system. At the expiration of the time mentioned, the dog was carried into another room, the tube was removed, and the ligature which had been around the trachea was divided by the scalpel, and withdrawn ; the animal after I had stitched up the aperture from which the tube had just been removed, breathed in the natural way. In this condition he was left, from 11 o'clock in the morning, until 6 in the evening, when his existence was instantly terminated, by thrusting a knife between the superior cervical vertebræ into the spinal marrow, with the view of procuring from his bladder the urine which had been secreted in the course of the day, it being retained by a ligature which I had passed around the penis, in the beginning of the experiment. After the urine was collected in a cup, Mr. Legaré and myself examined it with great attention, but neither of us were able to detect in it the least smell of violets.

The result of this experiment convinced me, that though the enquiries of Bichat and Rosseau had been incompletely conducted, yet the inference which others deduced from them was an important truth. But what establishes, beyond a doubt, the absorption from the lungs, is the following experiment.

EXPERIMENT II.

A strong dog being fastened down upon a table, an incision as in the former experiment, was made, just above the superior extremity of the sternum, and at this place, a ligature was passed around the trachea, which completely prevented the animal from respiring through his mouth; a small aperture was then made, below the ligature, into the wind-pipe, to which was adapted one end of a long tube, while the other end communicated with the mouth of a bottle containing spirits of turpentine. In this situation, the dog continued to inhale, by his lungs, emanations of turpentine, for two hours, the operation was then discontinued, the tube was removed, together with the ligature about the trachea, and the incisions which had been made were closed. The mouth, fauces and trachea, of course, resumed their former functions. The dog was now left, from 11 o'clock, A. M. to half after six in the evening, when he was killed: the urine collected gave a strong smell of violets. My obliging friend Mr. Thomas Smith, and myself, made ourselves acquainted with this circumstance, from repeated examinations.

The above two experiments will, if I am not mistaken, convince the candid reader that the lungs afford an extensive surface for absorption, and at the same time prepare him to expect the manner in which I shall prove that certain articles applied to the skin, gain admission into the system.

As most of the experiments made by different physiologists, in proof of absorption from the external surface, are exceptionable, on account of their not having been conducted in such a

manner as to exclude the lungs from an agency in the case ; it occurred to me, that experiments made over quicksilver, in inverted vessels, would be void of such, or any other objection. As spirit of turpentine readily insinuates itself into the system, and gives the urine a peculiar odour, which is a good criterion of its presence, I determined to make use of it, as the principal article in my experiments ; accordingly, in

EXPERIMENT III.

My foot was placed in a wash-bowl, containing spirit of turpentine ; friction was occasionally used, and at the expiration of one hour, as I experienced some pain from the irritation of the turpentine, my foot was withdrawn. In a short time I retired from my room, and did not return until the expiration of one hour, when the urine was found to be impregnated with the smell of violets ; the odour, however, was much more powerful at subsequent examinations. In the course of half an hour, from the beginning of my exposure to the emanations of turpentine, a slight head-ache, with an evident acceleration of the pulse, were excited, which continued for several hours ; but the urine continued to emit the smell of violets until the next day.

In this experiment, it is very clear that the spirit of turpentine was conveyed into the system ; but to ascertain whether the absorption took place from the skin or the lungs, the two following experiments were made.

EXPERIMENT IV.

Three days after performing the above experiment, a quantity of good spirit of turpentine was passed up into a glass vessel, previously filled and inverted in quicksilver. The cork in the mouth of the phial containing the turpentine, was drawn, under the surface of the quicksilver, so that the vapours of this volatile liquid, could not mix with the air of the room. My hand and wrist were next introduced into the inverted vessel,

and in this situation, surrounded by spirit of turpentine, were retained for an hour and a half. My hand being immediately well washed, I left the laboratory and walked to a considerable distance ; in the course of an hour, the urine was attended to ; but the smell of violets was not in the least observable ; the same reference was made repeatedly during the remainder of the day, without being able to detect any change in the natural smell of the urine. The systematic affection which was experienced in the former experiment, did not occur in this. I continued to inspect the urine, occasionally discharged, until the next day, but nothing characteristic of the presence of spirits of turpentine was detected.

This experiment was conducted under circumstances which allowed every opportunity for absorption to take place ; friction was repeatedly used by rubbing my hand and wrist against the sides of the vessel which confined the turpentine over mercury ; and what I deem of great importance is the complete manner in which the lungs were excluded from an interference which they would otherwise make in the experiment. Having now ascertained that in my first experiment the turpentine which was evidently carried into the system, did not gain access through the agency of cutaneous absorbents, it was natural for me to discover whether the lungs, which with the skin were the only parts exposed to its emanations, had not been the organs which accomplished the absorption.

EXPERIMENT V.

A glass vessel, containing a quantity of atmospheric air, was inverted in quicksilver ; three or four ounces of spirits of turpentine were introduced into it, and agitated with the air contained in the vessel, in such a manner as to intimately mix the odour with every part of it. A convenient glass tube was then used, one end of which communicated with the air in the vessel, and the other end was taken into my mouth, and in this manner I inhaled the air highly charged with turpentine,

without suffering any of it to come in contact with the skin. Having effected this part of my experiment, I left the room and took a moderate walk ; I had not retired long, before a slight head-ache and quickness of the pulse were experienced ; the urine voided, after an hour and a half had elapsed, was found imbued with the smell of violets ; but in the course of the afternoon, the impregnation was much greater, and which continued to be observed until 11 o'clock, P. M. when I went to bed ; the next morning the urine emitted only its usual smell.

The above experiments fully establish, in the first place, that spirit of turpentine is not absorbed from the external surface ; and in the second place, when it manifests itself in the system by the effect which I have mentioned it produces in the urine, consequent to its application to the skin, or diffusion through the air, it has been absorbed by the lungs and not from the skin, as some Physiologists have erroneously imagined.

It appears some-what remarkable to me, that men most refined in their researches in medicine, and who have otherwise greatly enlarged our knowledge of the animal body, should attribute absorption to the skin, without extending their enquiries to collateral circumstances, which must infallibly end, either in refuting or establishing their doctrine.

If spirit of turpentine, garlic, camphor, asparagus, &c. and the gases, are taken up from the external surface, why have they not been found in some part or other, of the lymphatic system ? In the most successful investigations that have been made, into the functions of the lacteal lymphatics, the nature of their contents has always been attended to, as affording a test of their having absorbed certain articles, previously presented to them. In answer to this, it may be said, that absorbents have not yet been discovered, opening on the skin of the

human body, while lacteals have long since been traced by the anatomist. But this temporary evasion involves a more glaring error ; for why contend, with so much assurance, for functions of parts whose existence is not yet proved ? If the skin was endowed, as the lungs are, with the power of absorption, the extensive surface of these two organs, would be a constant source of disease in the system.

With a view to the general subject of absorption, and to ascertain, in particular, whether camphor is taken up by vessels of the skin, as Professor Monro contended for, in his experiments upon frogs, I made the following

EXPERIMENT VI.

Eight ounces of a strong solution of camphor, in spirit of wine, were transmitted, with all the necessary cautions of experiment 4th, into a glass vessel, previously filled and inverted in quicksilver ; my hand and wrist were then introduced into the vessel, and retained in this situation, surrounded by the camphorated solution, for more than an hour. In the beginning of the experiment, 15 grains of nitrat of potash were taken, with the intention of encreasing the natural action of the kidneys, and thus affording a better opportunity of accomplishing the object in view. Friction, as in experiment 4th, was occasionally used, while my hand and wrist were held over mercury, in the solution of camphor. But, with all these measures to excite absorption, not a single circumstance occurred, either in the course of the day, or ensuing evening, which induced me to believe that this effect had taken place.

As it is a fact, familiar with the most of observers, that both the flesh and milk of animals which feed on garlic, are affected with the taste and smell of their aliment, I entertained no doubt, but that a strong infusion of this odorous substance, applied to the skin, under the circumstances of the following ex-

periment, would throw some light upon cutaneous absorption, taking the state of the urine and breath, as a proper criterion.

EXPERIMENT VII.

A quantity of a strong infusion of garlic was made, a portion of which, with great care, was passed up into a glass vessel, previously filled and inverted in quicksilver : my hand and wrist were then introduced into it, and retained in this situation for one hour. The infusion was carried to the laboratory in an eight-ounce phial, the cork of which was drawn, under the surface of the quicksilver, so that the air which I breathed was not in the least impregnated with it. Friction, as in the former instances, was frequently made use of. Upon withdrawing my hand from the inverted vessel, I instantly left the room, and having washed my hand with soap and water, I exercised myself in walking, for half an hour, and returned home. The urine and breath were then referred to ; in neither of which, could the smell of garlic be perceived ; my attention to this part of the experiment was not here discontinued ; for examinations were repeatedly made, during the remainder of the day ; but the smell of garlic did not in any instance, occur.

The facility with which absorbing vessels of the alimentary canal take up garlic, is a convincing proof, that this substance is not unfriendly to the appetency of the lymphatic system. If then, the skin of the human body is endowed with the power of absorption, is it not probable, nay, I will venture to ask, is it not certain, that this diffusible substance would have been conveyed into the system, in the above experiment ?

As the presence of asparagus, in the system, can always be known, by attending to the state of the urine, I resolved to ascertain, whether or not, this article could excite absorption from the external surface.

EXPERIMENT VIII.

A quantity of a strong decoction of asparagus was, with necessary cautions to prevent its odour from passing into the air of the room, transmitted into a glass vessel, previously filled and inverted in quicksilver. My hand and wrist were next introduced into the inverted vessel, and kept in this situation surrounded by the decoction for more than an hour; during the greater part of which time, friction was used, with the intention of giving every opportunity for an absorbing power on the skin, to exert itself. At the expiration of the time mentioned, my hand and wrist were withdrawn, when I directly left the laboratory, and in the course of one hour and a half, the urine was examined, but the peculiar odour which asparagus gives, could not be perceived. As in the former instances, I continued to attend to the urine occasionally discharged, during the remainder of the day; but nothing was discovered which characterised the presence of asparagus in the body.

I have now completed the detail of the experiments and observations which have given rise to the opposition which I have here offered to the sentiments of many respectable physiologists, on the functions of the skin of the human body: I am, nevertheless, conscious of having left unrefuted, many arguments, drawn from analogical data, which, slender as they appear to me, may notwithstanding, tend to lessen that conviction in the minds of some individuals, which it has been my endeavour to produce.

An enquiry into the functions belonging to the external surface of the different orders of animals will yet, I trust, though much light has already been shed upon this important subject, afford an interesting page in the annals of medicine.

I have for some time past observed the want of such a work, and if circumstances do not mar my present expectations, I intend to devote a share of my future liesure to the collection of materials for its publication.

To Dr. Samuel Stringer, of Albany, I am much indebted for useful instruction conferred in the beginning of my studies; and in return for which he will please to accept of my thanks and unalterable esteem.

Before I close this imperfect essay, it remains for me to proffer my thanks to the Medical Professors of this University, for the permanent advantages which I have derived from their instructing lectures, and for the polite attention which they have individually manifested towards me. That they may long continue to exercise the duties attached to their respective departments, with the same reputation that has heretofore characterised their labours, is the sincere wish of their obliged and grateful Pupil.

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